THE HARVEIAN ORATION

DELIVERED AT THE

ROYAL COLLEGE OF PHYSICIANS

JUNE 26, 1869

BY

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LONDON:

LONGMANS, GREEN, AND CO.

1869.

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With an imperfect conception of the magnitude of the task, I undertook the delivery of the Harveian Oration; and, on becoming a more diligent student of the works of Harvey, the little confidence I had felt in my capability fairly deserted me, and my anxiety wellnigh amounted to despair.

I would crave your forgiveness for my temerity, and your indulgence for my treatment of a subject which, for its full illustration, demands not merely an acquaintance with its history, but the power of appreciating the high moral qualities by which the discoverer of the circulation supported himself under the many trials of an eventful life—trials resulting from the contact of greatness with the envy and littleness of mankind.

Endowed with a powerful intellect, William Harvey approached the study of nature with all the fervour and curiosity of an ardent lover of truth. Self-reliant, and indulging a laudable scepticism, he fearlessly questioned where others dared not even doubt.

With the rare gift of discerning truth far below the horizon limiting the intellectual vision of the many, he combined the industry necessary to develope his views and enforce them upon the attention of the profession.

Notwithstanding these advantages, we find it was with him as with nearly all discoverers, for ridicule and persecution did much to embitter the existence of this benefactor to the human race.

Worthy of the high mission confided to him, his onward course was doubtless assisted by the mental exaltation ever attendant upon discovery, and, while we deplore the wounds inflicted by jealousy and malice, we may yet rejoice in the belief that many stirring sensations of delight must have acted as a healing balm upon his noble heart.

Harvey had, indeed, his full share of troublesome opposition; but we must not forget that other

trials, differing in kind, ever await the man of original thought whose innate love of truth carries him irresistibly forward in its pursuit. If, on the one hand, we are to believe in the great privileges enjoyed by the discoverer, we are on the other forced to acknowledge that the gifts of a vivid imagination and of great power of analysis are *in themselves* causes of trouble to the possessor. How frequently this self-inflicted pain must be undergone! The suggestive fact which cannot be made to take a place—the apparent antagonism of two equally well-ascertained truths—the sudden flight of thought towards generalisation meeting encouragement, and making the heart tremble with hope, and then the godlike power of analysis striking in, and casting down the cherished offspring of fancy as a mass of error and deceit! But the busy brain must still work on; the call will still be heard, and the reverie and the disappointment recur again and again.

So suffered Newton, and, in our own time, Davy. I heard one of the intimates of the last-named philosopher observe, that on certain occasions he felt sure Davy's mind must have failed him, had he not been torn from his pursuits by friendly

hands, and taken into the country to indulge in his favourite sport, but that even there he could not keep his mind from wandering back to those subjects which had now become torture.

It must be confessed, however, that we have to consider the other view of the case—the triumph of success. The man in whom genius is rendered fruitful by labour and by the corrective influence of the understanding, has most probably within him a source of happiness and contentment unknown to the common world. We can only guess at this, and gladly believe in its extreme value. We may perhaps, however, form some indistinct idea of the philosopher's gratification when each devious path which in turn had led to error is recognised as a snare—when supposed facts have their true nature detected, and fresh observation has cast light on the true bearings of collected instances—when suddenly the whole truth is grasped, and all is clear.

When reflecting upon the history of discovery, we cannot fail to be impressed by the fact that in many cases discoverers have been greatly assisted by the labours of others—that the progress of science has brought about the discovery so nearly

as a matter of necessity, that we need not regard the crowning effort with admiration and surprise. We who are in full possession of the great truth enunciated by Harvey, are totally incompetent to estimate the full amount of difficulty he had to encounter. It is true, when we read the works of his predecessors, we cannot but observe how nearly the circulation of the blood is therein described, and we might at first be inclined to believe that he deserved less credit than has been apportioned to him; but a moment's reflection serves to show how impossible it would have been to set aside the vast mass of error which shut out the light from all ordinary capacities, save by the exercise of great originality and power. This is clearly brought before us in the difficulty which was experienced in gaining credence for the discovery of the circulation, even when backed by the most conclusive experiments.

Sir William Temple, writing many years after the discovery of Harvey had been given to the world, alludes to the doubt which still affected the minds of physiologists. This accomplished writer says:* 'Dr. Harvey gave the first credit, if not rise,

* Works of Sir Wm. Temple, vol. iii. pp. 293–469 (A.D. 1673).

to the opinion about the circulation of the blood, which was expected to bring in great and general innovations into the whole practice of physic, but has had no such effect. Whether the opinion has not had the luck to be so well believed as proved—sense and experience having not well agreed with reason and speculation—or whether the scheme has not been pursued so far as to draw it into practice, or whether it be too fine to be capable of it, like some propositions in the mathematics, how true and demonstrative soever, I will not pretend to determine.' Again, Sir William goes on to say: 'There is nothing new in astronomy to vie with the ancients, unless it be the Copernican System, nor in physic, unless Harvey's circulation of the blood. But whether either of these be modern discoveries, or derived from old fountains, is disputed—nay, it is so too, whether they are true or no, for though reason may seem to favour them more than the contrary opinions, yet sense can very hardly allow them, and to satisfy mankind both these must concur. But if they are true, yet these two great discoveries have made no change in the conclusions of astronomy nor in the practice of physic, and so have been of little use to the

world, though perhaps of much honour to the authors.'

These remarks clearly show that Harvey's discovery was regarded with suspicion long after his death, and there can be no better proof than this affords of the great truth having been unsuspected by his predecessors, notwithstanding the remarkable passages which are to be found in the works of older writers. It is certainly most interesting to observe how nearly the circulation was described by some of these, and how centuries elapsed without a single mind awakening to a full perception of the subject.

It was not that the authors were neglected; on the contrary, their works were among the class-books of the day, and their very words prized and commented upon. With the knowledge we now possess, it is difficult to understand how it happened that so little effect ensued from an acquaintance with the words of Aristotle, which were known to physicians two thousand years ago. He wrote as follows: 'The blood of all animals palpitates in their veins (meaning arteries), and by the pulse is sent everywhere simultaneously;' and, further, he adds: 'Thus do all the veins

pulsate together, and by successive strokes, because they all depend upon the heart.'* These words produced no impress on the physiology of the day, and we find authors wandering darkly around the great fact which it was Harvey's mission to bring to light. For eighteen hundred years after Aristotle, no advance was made worthy of note; for Servetus, who wrote in 1553, and of whose labours too much has been made by commentators, did little more than offer a conjecture on the pulmonary circulation. It is worthy of notice, however, that at this date the necessity for contact of atmospheric air with the blood coming from the right ventricle was recognised.

It was by this contact, the Vital Spirit, as it was called, was produced or engendered: the colour of the blood changed to crimson, and, purified by the inspiratory act from fuliginous vapours, the vital fluid became fitted for the preservation of life and health. The blood of the right ventricle was known to require great purification, and it is to be hoped physiology will soon help us to a more complete knowledge of the changes effected by the respi-

* Willis's Translation of the Works of Harvey (Sydenham Society).

ratory act than that which we now possess—for we speak very much more as Servetus did than could be desired in the nineteenth century. We deal with little else than fuliginous vapour, though our acquaintance with gaseous chemistry enables us to call it by another name.

There is scarcely anything more remarkable within the whole range of physiology than the great and rapid change effected by respiration. When we consider what are the contents of the right auricle of the heart—how it can scarcely be said to contain blood—it is indeed surprising that the wondrous fluid, the great builder of the organism, can be so rapidly formed from such materials.

Here we find chyle and lymph freshly mixed with venous blood poured into the cavity. These fluids have now to mix with blood from the hepatic veins, charged with a peculiar animal principle, and these materials are to be worked up into something fitted for circulation through the lungs, where, by contact with atmospheric air, a fluid is elaborated having a definite and fixed constitution; definite in the quality, and fixed in the proportion of its constituents.

I have said that Servetus scarcely did more than offer a conjecture on the pulmonary circulation. In 1559, however, Realdus Columbus* made so broad and comprehensive a statement regarding the circulation, that we must allow him to have been so near the truth that a little more independence of thought and experimental constructiveness must have given him the glory which brightens the memory of our illustrious countryman. Be it remembered, however, that it was precisely because he lacked these qualities that he could not be that which Harvey became—the discoverer of the circulation. How strongly to the point are the following words of Columbus: 'The blood, when once it has entered the right ventricle from the vena cava, can in no way again get back; for the tricuspid valves are so placed, that whilst they give a ready passageto the stream inwards, they effectually oppose its return. The blood continuing to advance from the right ventricle into the vena arteriosa or pulmonary artery, once there, cannot flow back upon the ventricle, for it is opposed by the sigmoid valves situate at the root of the vessel. The blood, therefore, agitated and

* De re anatomicâ (1559).

mixed with air in the lungs, and thus in some sort having acquired the nature of spirit, is carried by the arteria venosa or pulmonary vein into the left ventricle, from whence being received into the aorta, it is by the ramifications of this vessel transmitted to all parts of the body.'* The learned translator of Harvey's works, to whom all Harveian orators must feel indebted, observes: 'This much looks very like an exposition of the circulation of the blood as understood at the present time.' And so it does indeed, but then we find that Columbus could not have had a correct idea of the manner in which the external stream was continued in the veins, and the circle completed; for we find him adopting the notion of the day, as to the origin of the veins from the liver, which is quite incompatible with a correct appreciation of the movement of the blood in a circle. And thus it was that the great truth became the prize of Harvey.

It may have been imperfectly conceived by others, suspicions of its true nature may have entered the minds of men, but Harvey made it stand forth, assume form, and show purpose. When

* Willis's Translation (Sydenham Society).

we consider how violently the views of Harvey were opposed, how he was persecuted and ridiculed, and that his practice left him owing to the mistrust of the profession and the public as to his sanity, we may indeed feel grateful that the circulation of the blood is even now an admitted truth, and that we are not deprived of the many and great advantages afforded us by a knowledge of the fact.

The admirably constructed mind of our great discoverer seems to have used in 1615* the method of inductive inquiry so completely systematised by Bacon in 1620. It can scarcely be too often repeated, that the publication of Bacon's great work did nothing more than exercise a corrective influence over the minds of scientific men; beyond this it was powerless, and the rare qualities which distinguished Harvey were not rendered less essential to the solution of the great problems of nature. The Baconian system may be invaluable to the laborious collector of facts, but it can neither give powers of observation nor originality of conception, and certainly cannot assist us to that enviable

* Harvey's Lectures to the Fellows of the College were given in 1615; his Work was published in 1628.

quality which sees analogies or points of difference undistinguishable by the vulgar.

It is vexatious to observe how frequently writers and talkers quote Bacon as the introducer of the inductive method of reasoning; the fact being, as Herschel states it, that 'it is not the introduction of inductive reasoning as a new and hitherto untried process which characterises the Baconian philosophy, but his keen perception, and his broad and spirit-stirring, almost enthusiastic announcement of its paramount importance.' It was Bacon who thus exposed the weakness of the Aristotelian philosophy, and substituted an extended scheme, glorious in its simplicity and startling in its perfection.

With all this it must be remembered, as Herschel has said, that 'the inductive method had been practised in many instances, both ancient and modern, by the mere instinct of mankind.' Hippocrates practised inductive reasoning, and we find Boerhaave, in his oration, 'De Commendando Studio Hippocratico,' written in 1701, insisting on this, and recommending the Hippocratic method of conducting medical inquiries.

Far removed from such perfection was the

method which next attracted general attention, for we find that Plato and Aristotle were permitted to establish systems utterly opposed to the method of Hippocrates.

It is impossible to reflect on the general character of Aristotle's works without feeling deep regret that a man, who evidently possessed much originality of thought, great industry, and who moreover had an extensive acquaintance with natural objects, should have done so much mischief by what has been well termed 'a daring spirit of contradiction and innovation.'

The superstitious reverence felt for the memory of those who have led mankind captive by the force of individual character bids me be careful in speaking of the career of this remarkable man; but it may be fairly doubted whether all we find in the wondrous mass of composition he has left us, treating as it does of almost every imaginable topic, had not better have been lost to us than that men should have been led astray by what is called the Aristotelian philosophy.

For my own part, I am content to believe with Bolingbroke, that Plato and Aristotle 'invented systems more baneful to truth and real learning

than the ravages of the Goths and Vandals.' And yet how powerful was the impress produced! The whole thinking world was fascinated, and it was not till the commencement of the seventeenth century that philosophers became freed from the thraldom of hypothetical propositions and syllogistic quibbles.

What an epoch was that seventeenth century! Springing from darkness, the mind seems suddenly to have become animated with an intelligence new to the earth. We have but to look to the century preceding Harvey, to be both surprised and amused at the state of science. It is scarcely agreeable to hear from the chronicler how medicine stood in those days. When that century was near its end, in 1595, when we here in England were greater than we had ever been, when Shakspeare wrote, when great men flourished, and the noble Elizabeth had good reason to be proud of the vessel of the State—whose helm she had handled with so much dexterity and courage—what was then the state of the mind medical?

We may judge of it from an instructive tale which we have on record. A golden tooth appeared in the jaw of a child in Silesia. This was

hailed as a miracle and an omen, and its meaning is thus given to us by the learned Dr. Horst. This eminent physician explains the phenomenon by telling us that at the birth of the child the Sun was in conjnnction with Saturn, at the sign of Aries; that the golden tooth was the herald of a golden age. The Doctor also says, the matter is clearly alluded to by Daniel, in his well-known second chapter, where the prophet speaks of a statue with a golden head. When things were thus, as years rolled on, sceptics and scoffers arose—rude men asking awkward questions, and some of these could be silenced by the frown of authority; but there were also, very fortunately, rude men of another kind—brave, bold men, who could not be frowned down—and all honour to them, for it is through them that we are free.

These men were considered wicked and unbelieving, but they had something within them that nourished their strength, something which whispered to them through days of toil and nights of thought, that their mission was high, for truth was inscribed on their banner; and while on the one hand they modestly believed wellnigh all things possible, on the other boldly refused to

believe anything whatever, unless good reason could be given for the adoption of the creed; and so in the seventeenth century our chains fell.

While purely experimental science made enormous advances after this great era, medicine by no means proceeded with equal stride.

With regard to the great discovery of Harvey, it may, indeed, be said that whether we regard pathology as shown us in the solids, or as it has advanced by the light of a revived and philosophic humoral pathology, it is only since the commencement of the nineteenth century that we have used our knowledge of the circulation to assist our progress.

Little was to be hoped from Harvey's discovery while the medical profession was fully imbued with the spirit of Galen's writings, when our books spoke of hot and cold remedies, and the cardinal qualities formed matter for grave consideration among the learned in physic; and this was the case long after Harvey's death. Hippocrates had adopted a method of inquiry far too laborious to please when contrasted with the wild system of hasty generalisation practised by those who followed him; and this taken in connection with the

fact that the physical sciences can only assist medicine incidentally, and often most indirectly, not only explains the position of the art immediately after the time of Hippocrates, but through every following century. It must not be forgotten that we have only just ceased to intersperse our language with that which we now regard as little else than jargon. It is scarcely credible, but nevertheless it is the fact, that so lately as 1711 we find an English physician translating a medical book written long after the discoveries of Harvey had been published, in which the learned translator indulges recklessly in hypothesis. He tells us how he selected Sylvius and Willis as patterns—blaming the former, however, for referring all the corruptions of the humours to the vicious effervescences of the acids and alkalies, and regretting the error of the latter in his attempted explanation of convulsive affections, which are so probably accounted for by the prevalence of an acid, or, to use his own quaint diction, 'some such grating substance for their cause.'

As to the author whose work he translates (the learned Etmüller of Leipsic), we find him endowing Ascarides with qualities indicating either a most

wondrous instinct or a profound acquaintance with astronomy. These worms, he says, observe the motions of the moon, and ought to be attacked when the moon is in the wane.

This was considered valuable matter by readers in 1711, for the work sold rapidly, and more than three editions were bought by the profession in a few years. If we now pass on from 1711 to a period nearly a century later, we find that the great discovery of Harvey had done but little for pathology, that it was still 'a scheme too fine to be drawn into practice;'* but yet great advances were making, facts were being collected and arranged, and all attempts to generalise were met by the most critical scrutiny.

Though we are greatly indebted to our knowledge of the circulation of the blood for much that modern science has done for medicine, still we cannot but allow that pathology would have been enriched by a vast amount of discovery had we been ignorant of the fact up to the present day. The pathology of nervous diseases might have been developed, and the discoveries of Bright, intimately connected as they are with the subject of blood

* Sir Wm. Temple. Op. ante cit.

change, might have been afforded us had we been in total ignorance of all that Harvey has written on the circulation of the blood and the motion of the heart; and the same may be said of much that has been done relating to the morbid states of many internal organs. If we go back to the time of Cullen, it clearly was regarded as something against which it was well to warn the student. That physician wrote the following memorable words in the year 1784 :—*

'The knowledge of the circulation did indeed necessarily lead to the consideration as well as to a clearer view of the organic system in animal bodies, which again led to the application of the mechanical philosophy towards explaining the phenomena of the animal economy, and it was applied accordingly, and continued till very lately to be the fashionable mode of reasoning on the subject.

'Such reasoning, indeed, must still in several respects continue to be applied, but it would be easy to show that it neither could, nor ever can be applied to any great extent in explaining the

* Preface to First Lines of the Practice of Physic, vol. i. p. 9.

animal economy, and we must therefore look for other circumstances which had a greater share in modelling the system of physic.'

So far as I have been able to determine the point, Laennec appears to have been the first pathologist to whose discoveries a knowledge of the circulation was absolutely necessary. Had not Harvey described the motion of the heart and its consequences, we never could have interpreted the phenomenon observed in cardiac auscultation.

It was Harvey who first clearly described the systolic and diastolic states, and thus enabled the great auscultator to deduce, almost as of necessity, the various consequences of valvular imperfection. Again, had we not recognised the circulation, pathologists must have remained in ignorance of the simple and beautiful subject of embolism and its multitudinous applications to the explanation of morbid conditions. Without the knowledge given us by Harvey, how full of mystery must have appeared those congestions which we now at once explain, or anxiously anticipate.

The liver which is to appear below the ribs as a consequence of cardiac imperfection—the fluid which is to fill the peritoneum as a consequence of

obstructed hepatic circulation—these, among other instances, crowd upon us to show that pathology as well as physiology has used the fact that the blood circulates.

It must ever be regarded as most surprising that Cullen could not explain the two congestions to which I have referred. Harvey's views, we must assume, so startled the medical world that it required more than a century and a half to bring pathologists to consider them properly.

We are now, however, in a position to deal with the subject, not only in its simple form, but to examine it in its relation to the nervous system, and surprising results have been obtained in this direction of late years.

I would especially allude to the experiments of Bernard and others, on the influence of the nerves on the capillary circulation—the vaso-motor power—affecting as it does both our pathology and therapeutics. Experiments made in order to examine further into this remarkable discovery have demonstrated that the nervous arrangement constituting the governing power includes cerebro-spinal as well as sympathetic branches, for which end nerve fibres pass from the medulla oblongata,

by the anterior roots of the spinal nerves, into the cervical portion of the sympathetic and into the splanchnic nerves.

In reflecting upon these very interesting manifestations of nervous force, we may be allowed to regret that we have not the advantage in the present day of applying the mind of a Harvey to the task of unravelling and developing, with the originality and boldness so necessary to the work, the nature and scope of the mysterious power transmitted by the nerves. We are checked, perchance, in dealing with this great subject, by the influence of some generally received but yet false notions, which the perspicacity of a Harvey might at once set aside, and with a mind thus freed from a weight of error, we might expect him to pass on to discovery with rapid steps. We know no more of the intimate nature of nervous force than our predecessors of the eighteenth century, and for my own part I would say, there appears but little hope of advancing our knowledge of the subject until the nervous system is regarded from other points of view than those from which it has been examined. The neglect of chemistry in its application to this subject is remarkable, and that its

relation is intimate and important can scarcely be doubted. We require to determine that an especial set of phenomena characterise the nervous force, and distinguish it from all other forces with which we have to deal; and I would venture to suggest that chemistry promises something in this inquiry. Even in its general bearing, we observe very interesting material for reflection. I may here allude to the fact (mentioned on a former occasion within these walls), that the cerebro-spinal and sympathetic systems have their distribution so arranged that the cerebro-spinal branches terminate in *acid* fluid, whereas the sympathetic nerve is distributed over surfaces having an *alkaline* reaction. I may add to this the striking fact, that the great sympathetic ganglia are strongly alkaline, while the brain and spinal marrow (and more especially the grey matter), if examined immediately after death, are scarcely neutral to test-paper, and become rapidly acid on exposure. I was enabled to establish these latter facts in the physiological laboratory of Guy's Hospital, where the semilunar ganglia of a dog were dexterously obtained for me by my friend Dr. Pavy. I cannot but believe that great good would accrue, if those

who have the time and the taste for inquiry would apply chemistry carefully and minutely to a subject which is not only deeply interesting to us as pathologists, but which more especially recommends itself to the student by the mysterious beauty of its manifestations.

If we now pass on to consider the position of humoral pathology in the present day, we see at once how necessary a knowledge of the circulation has been to its advancement. Dependent as this branch of science is for its successful prosecution upon a combined knowledge of chemistry, physics, and practical medicine, but few competent labourers can be found in the field, while the importance of the inquiry far exceeds that which, as physicians, we ought to attach to any other branch of pathology. When reflecting on the morbid changes effected in the solids, whether they be such as to admit of detection by the unassisted eye, or to require the aid of that valuable, though in some hands dangerous instrument, the microscope; still, we are obliged, if we desire to advance but a single step, to revert to the probable changes in the fluids which have preceded and brought about the lesion. Trite as this remark may appear, I am

moved to make it here from a conviction that the truth it conveys cannot be placed too strongly before the minds of pathologists. We ought certainly to regret anything like indifference on this subject, but I would submit that when a certain set of symptoms have been connected with the existence of some especial solid lesion, we do not always find that eager yearning to become informed of the preceding fluid changes which would inevitably result from a deeper conviction of the great value of such knowledge. It is true that science has not done much to assist us here; but still some little has been afforded both by the physicist and chemist which deserves more attention than has yet been devoted to it. I would adduce the remarkable observations made by Graham on the diffusion of fluids through membrane. These are so suggestive that we have every reason to hope we may apply the facts he has ascertained to the explanation of many intricate problems. His arrangement of bodies into the two classes crystalloids and colloids, having strikingly different qualities in relation to membrane, necessarily suggest the application of the discovery to pathology.

I may here instance the occurrence of dropsical

effusions which we cannot attribute to obstructed circulation, such as the effusions in Bright's disease.

When we remember how urinary salts and urea (which may be regarded as the crystalloids of the urine) foul the blood in albuminuria, causing a change in the qualities of the liquor sanguinis, by the introduction of an excess of substances capable of permeating membrane with facility, we at once see how a knowledge of Graham's law may possibly assist us in explaining the dropsical state.

The loss of the extractive matters of the blood may also be cited as tending to alter the relation of the liquor sanguinis to the membranes with which it comes in contact. In Asiatic cholera, when the disease is characterised by the usual flow of serous fluid from the intestinal surface, conditions are present suggesting the probability that retention of crystalloids in the circulation may have much to do with bringing about the result; for, if the secretions generally, and especially the secretion of urine, be suddenly stopped, the blood will become rich in crystalloids—a condition which we know greatly favours the diffusion of fluids through membrane.

In making these remarks, I would not desire to give more than due importance to this law in its application to the condition of membrane while constituting a portion of organised beings.

There is much danger in this temptation, for though it is difficult to believe but that these forces, manifested so strongly in dead matter, have their influence under what we must call conventionally vital conditions, still we are bound to wait patiently for further observations and discoveries before we can presume to push forward theory from such a basis. We see the necessity for this caution in the fact, that living membrane in some cases possesses qualities under the conditions of health which apparently render it capable of resisting the action of this law; and therefore, in explaining the phenomena of disease, we must carefully remember that we probably have to deal with other and modifying forces, concerning which we are, as yet, uninformed. Thus, it is our duty to keep in mind the possibility of applying every physical fact which relates to our science, and to watch anxiously for the moment when it may become useful; but, at the same time, not to lose sight of the varying conditions of the materials with which we have to deal, and of the

possible influence of the forces concerned in the operations of the organism. To do more than this (which has been too often the case) is as mischievous as it would be, were we to refrain from suggesting the probable utility of a discovery having a strong bearing on our subject, merely because it was incapable, as it stood alone, of explaining satisfactorily any given phenomenon. It is to be hoped that collateral experiment, bearing on Graham's law, may early bring us to important results.

In considering the application of chemistry to the pathology of the fluids, we meet with a field of inquiry so vast that, notwithstanding the labour and thought which have been bestowed upon it, it is comparatively unexplored. The pathological student here finds himself involved in the study of very various and very confusing questions, having reference both to the proximate and ultimate analysis of animal matters.

The normal standards given him by the chemist are to serve as starting-points of inquiry, but in applying all this knowledge to pathology he will rarely gain much advantage. It may happen his faith becomes less firm as his acquaintance with the subject increases, and that he is scarcely bold

enough to acknowledge an infidelity which might bring obloquy upon him. Though checked by these considerations, the student may be cheered by the reflection, that the true objects of pathological research are rarely appreciated by the chemist who is not a worker at the bed-side. Symptoms must be correctly noted and valued before they can be usefully connected with changes from the healthy constitution of the fluids, and this is no task for those who are unaccustomed to look upon disease. On the other hand, the physician, unless he be an accomplished chemist, will not comprehend the true nature, bearings, and value of the results obtained in the laboratory. The chemist's return sets forth the proportion of various proximate elements contained in the material submitted to analysis; but that this yields any information worth possessing to the practical physician who is not a chemist, is to my mind very problematical.

Nothing is more to be desired than that pathologists should become practical chemists, so that when pondering over these difficult questions they may be able to apply chemistry with facility, and thus avoid that disruption of the train of thought or reasoning so fatal to discovery. But we must

not suppose that chemistry has done nothing for us. In its application to the detection of symptoms it has been very valuable. For instance, Bright never could have determined the point he has established, had he merely used the test of heat in examining urine for albumen, and his first tables were constructed on that plan. We all know now that heat precipitates phosphate of lime, as well as albumen, in a multitude of instances, and therefore that these tables would have shown that coagulable urine (as it was then called) constantly occurred without the concomitant lesion of kidney. Bright's discovery might thus have been left as a prize for some other observer, whose chemical knowledge enabled him to apply a more perfect method of examination. Fortunately, Bright's tables were reconstructed, a correct chemical method of determining the presence of albumen was adopted, and the exceptional cases which threatened confusion disappeared from the return. Here, chemistry, by determining a symptom, helped us to one of the most important pathological discoveries of modern times. But its minor services of this kind are by no means despicable. We are all familiar with its use in

diabetes, in jaundice, and in many forms of urinary disease, and we shall doubtless use it more and more as pathology advances. In considering the value of chemistry as applied to the analysis of the fluids, we must not forget its importance when used as an interpreter of the true nature of the results of disease as seen in the solids. I would submit that the microscope has been too exclusively relied upon in prosecuting this interesting inquiry; that form and arrangement have too much engrossed the attention of pathologists, while the chemical characters of the materials under examination have not received due consideration. We require more numerous analyses of diseased solids, and a combined chemical and microscopical method of examination. Nature works in part chemically, and it is obviously conducive to error to be contented with a knowledge of physical qualities alone.

The microscope, except it be used to assist us in detecting the minute dissections (if I may be allowed the term) practised by the application of chemical re-agents, loses half its value; and whenever physicians become chemists as well as micrographers, we shall very probably see a host of

errors, now received as truths, swept from our path, and the atmosphere cleared for the perception of facts now obscured for want of the proper light by which to discern their nature and value.

Much might be said of the abuse of physiology in its application to pathology. We are not quite secure in the present day from the tendency to too rapid generalisation which infected men's minds in Harvey's time, and which contrasted so strongly with his mode of thought and procedure. We may perhaps attribute this evil partly to the state of the medical profession, which in these days of competition has become greatly troubled. It is now the fashion to work, and many do so because they are ashamed to be thought idle. If naturally slothful, a fact or an assumed fact supplies them with sufficient matter for a theory, and then with little trouble they can appear before the public at frequent intervals, borne on a cloud of misty composition. Or, they may be more industrious, but lack wit, and urged forward by the inspiration of example, go through much labour.

But they have worked badly, and find a very small return for the time and trouble expended.

This, however, does not always deter them from generalising, and thus it is that now-a-days, while more work is done, less good work is done proportionally than was effected by the smaller number of labourers of the last century, who rarely worked unless they felt strong inclination and possessed ability.

In reflecting on the present we may be allowed to hope for the future. Some may rejoice in believing that those who are to follow us enjoy great advantages, and will become more competent labourers than their predecessors. In this wondrous microcosm with which we have to deal, with its valves, levers, streams, and springs; with its mysterious force exercised in such manner that analogy scarcely assists us in the attempt to unravel its nature; in this abode of wonders, doubtless every discovery in physical science finds its application. Let us see in this reflection the advantage of educating the rising generation, so that they may at least be competent to understand and apply the observations of those who work for them; for all who work at physical science must work for the physician. I would humbly submit that the medical education of the present day can scarcely be con-

sidered very valuable so far as regards the advancement of medicine as a science. One of the great evils we have to contend against is want of time. Owing to this, the present curriculum is most overwhelming to the student, who probably might be able to fulfil the conditions required in double the number of years now allotted to study; but who is forced, under existing regulations, to acquire an imperfect acquaintance with a multitude of subjects.

This is not a bad scheme for retarding the progress of medicine. Fortunately, human nature rebels against the imposition of unreasonable conditions, and the student unwittingly uses in his defence a quality of mind, the existence of which should never be forgotten by those who legislate on the important subject of education.

Why it is so I know not, but long experience as a teacher and examiner has shown me that the mind in acquiring knowledge becomes active in two very different ways. It may act in such manner that the impressions received are of permanent character, or it may learn a lesson (and very correctly too) which will be dismissed from the mind so soon as it has answered the required end. The latter mode of engaging thought enables the

student to pass creditably in subjects which he can never make use of in after life, for he has not acquired them so as to give them a permanent place in his memory. This great difference in mental action appears prominently in forensic practice, where it is much required, but the less we have of it in the study of science the better.

We cannot regret, however, that the student adopts the less perfect form of acquiring that part of education having reference to science generally which is enforced by the curriculum. He has been enabled, by so doing, to devote more time to the acquirement of knowledge immediately bearing on practice, and has had, therefore, the opportunity of more fully digesting facts, and more carefully arranging his thoughts, than could have been the case had he acted otherwise.

The few who have both time and taste for study, or who, gifted with extraordinary talent, can acquire in three or four years that complete knowledge of science now demanded of the student by the examiner, may well be envied. The great majority, however, are devoid of such taste, and will fail to benefit from scientific teaching, however carefully it may be applied. These

latter become very valuable in the daily drudgery of professional life. They may possibly rise to the position of useful, eminent, and fashionable practitioners, but the scientific training through which they passed while in the schools did little more than interfere with the acquirement of that empirical knowledge which was the only thing their minds were competent to deal with.

There is great evil in enforcing scientific studies on the attention of the student who has but three or four years to devote to the acquirement of practical knowledge.

If by doing so you could make him love science and follow it, then, indeed, a glorious result might follow; but that is impossible. Men become labourers in science only from innate love of it; but the privilege is for the few, and the student during his short stay in our schools should not be distracted from the acquisition of practical knowledge by the futile attempt to make him a scientific man.

If he possess the taste, he will follow science when he has left us; he will become one of those who can understand the results of the physicist and chemist, and perhaps, by labouring to advance

scientific medicine, will secure to himself a source of never-failing enjoyment.

It may, indeed, be regarded as a great privilege to be allowed to work, with however little success, in this beautiful field of inquiry, where from time to time the veil which hides so much from us seems failing in its office; where truth (as it often does when half concealed) assumes an ever-varying form, playing with thought, and pleasing fancy, even when tempting us far from the paths of discovery.

It must have been a great source of happiness to Harvey, when, as the historian says, he 'lost his practice' and 'was thought crack-brained,' that he had left him the purest and highest of all pleasures, the appreciative contemplation of a subject rich in wonders and fertile in great results.

It would appear, and more especially so from the passages I have quoted from Cullen, that the dread of a new system of treatment had much to do with the success of Harvey's detractors in depriving him of practice. The public would naturally regard with anxiety the advent of a new therapeutical era having an experimental character which the profession generally considered incon-

sistent with safety. They might well be expected to fear that the older remedies, the value of which had been tested by the experience of ages, would fall into disuse, and that practitioners would thus be left comparatively helpless against the incursions of disease, instead of armed with their old and well-tried weapons of defence. The lapse of a few years served to remove all anxiety on this point. It was obviously premature to anticipate any change of the kind from Harvey's discovery, inasmuch as therapeutics up to that date had never been materially modified by advances in physiology.

We frequently hear it said that our profession neglects the study of therapeutics, and there is, I apprehend, too much truth in the statement. Whence this indifference? It can scarcely be that we underrate the value of the subject, for it is incomparably the most important study comprised in the curriculum of our schools, and without it all medical knowledge is as nearly as possible useless.

May we not regard this neglect of therapeutics as a result of the extreme difficulty attending the prosecution of the study? It can only be scientifically followed by those who are accomplished

diagnostics, and who moreover have been accustomed to investigate physical phenomena, and, having thus undergone the necessary tuition, are the less likely to be led astray by false appearances and the influence of preconceived ideas. At several different periods in the progress of our art, it has been proposed, as a means of advancing therapeutical knowledge, that we should endeavour to ascertain the physiological action of drugs, and then apply them in order to repress or modify pathological activity. This method was adopted by Philenius and Serapion 300 years before Christ, and has been followed up from time to time since that date with some advantage.

The instances in which we have been thus assisted are by no means few, and I may mention, as a recent adaptation of a physiological observation to pathology, the use of sedatives as applied in accordance with our knowledge of their physiological action on the vaso-motor nerves. Notwithstanding the value of this method of procedure, it fails us in numerous cases, and we are constantly thrown back to experiment on the sick, as the more direct and the universally decisive plan of action. It is to this purely empirical

method that we owe the discovery of most of our highly prized drugs, especially the remedies known as 'specifics,' nearly all of which have been made known to us by mere accident. Thus quina, iodide of potassium, bromide of potassium, and colchicum, which in certain diseases we prescribe with great confidence, came before us having no claim upon our attention beyond the fact that certain persons had tried them as remedies without being able to give any good reason for having done so. It is very interesting to watch the efforts making, as science advances, to cast light on this great question of therapeutics.

We find chemical philosophers interesting themselves in attempting to trace a relation between the physiological action of a substance and its chemical composition and constitution. Chemical composition scarcely helps us, however; for though it has been shown that salts of the same base occasionally resemble each other in their action upon the economy, still the opposite is so often the case that we cannot hope for much assistance here. A fact mentioned by Bunsen is again remarkable in its relation to this subject. That observer has shown that kakodylic acid, which contains more

than 54 per cent. of arsenic, is inert, although perfectly soluble. In a paper of great merit, read before the Royal Society last year, we find Dr. A. Crum Brown and Dr. Thomas R. Fraser entering upon this branch of inquiry in a very philosophical spirit. After showing how impossible it is to connect physiological action with *composition*, they proceed to inquire whether light may not be thrown on the point by taking into consideration the chemical *constitution* of substances—defining constitution as 'the mutual relation of the atoms in a substance.' By an ingenious method of directing experiment, into which I cannot venture to enter here, the authors have arrived at some remarkable results, which may probably at no very remote date give some assistance to the therapeutical student. Whatever may be done, however, by the assistance of chemistry and physiology, we must not underrate the value of the purely empirical method. It is most excellent when carefully applied by persons conversant with disease. The great evil attaching to it is that those who are not competent to determine pathological conditions occasionally addict themselves to therapeutical inquiries. It was thus with Hahnemann, who experimented most

industriously, but whose work indicates in the most unequivocal manner his incapacity to determine the nature of the pathological conditions which he attempted to combat with his remedies.

There was then but little reason in the view that therapeutics would be affected by Harvey's discovery; but we need not be surprised that the belief was encouraged, and that he lost his practice.

The honourable position he occupied angered the ambitious—his bold honesty offended the deceitful, and his growing fortune tempted the covetous.

And now we find him complaining, not, indeed, of decreasing wealth, but of the unworthy treatment received from those who ought to have loved and protected him. In his work on Generation * we find the following lines: 'And whilst I speak of these matters, let gentle minds forgive me, if recalling the irreparable injuries I have suffered, I here give vent to a sigh. This is the cause of my sorrow: whilst in attendance on his Majesty the King, during our late troubles and more than

* Willis's Translation of Harvey's Works (Sydenham Society), p. 481.

civil wars, not only with the permission, but by command of the Parliament, certain rapacious hands stripped not only my house of all its furniture, but what is subject of far greater regret to me, my enemies abstracted from my museum the fruits of many years of toil.'

It was left for succeeding generations to mourn over the troubles of this great master, and to appreciate the worth which brought upon him the vengeance of his contemporaries.

The memory of the great discoverer has now a place in the heart of every lover of truth. He lived to surprise the world by the display of rare gifts, and it is for me, standing within these walls, decorated by the work of his honoured hands, reverently to ask your loving remembrance of one whose labours are, to us, an eternal monument of greatness, and who now, numbered among the mighty dead, bids us, by his high example, to go onward hopefully and fearlessly to the last.

LONDON : PRINTED BY
SPOTTISWOODE AND CO., NEW-STREET SQUARE
AND PARLIAMENT STREET